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EXPERIMENTS ON ANIMALS

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Experiments on Animals

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PART I
EXPERIMENTS IN PHYSIOLOGY

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EXPERIMENTS ON ANIMALS

I
THE BLOOD

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I.—Before Harvey

Galen, born at Pergamos, 131 A.D., proved by experiments on animals that the brain is as warm as the heart, against the Aristotelian doctrine that the office of the brain is to keep the heart cool. He also proved that the arteries during life contain blood, not πνεῦμα (Greek: pneuma), or the breath of life:—

"Ourselves, having tied the exposed arteries above and below, opened them between the ligatures, and showed that they were indeed full of blood."

Though all vessels bleed when they are wounded, yet this experiment was necessary to refute the fanciful teaching of Erasistratus and his followers, of whom Galen says:—

"Erasistratus is pleased to believe that an artery is a vessel containing the breath of life, and a vein is a vessel containing blood; and that the vessels, dividing again and again, come at last to be so small that they can close their ultimate pores, and keep the blood controlled within them;

yea, though the pores of the vein and of the artery lie side by side, yet the blood remains within its proper bounds, nowhere passing into the vessels of the breath of life. But when the blood is driven with violence from the veins into the arteries, forthwith there is disease; and the blood is poured the wrong way into the arteries, and there withstands and dashes itself against the breath of life coming from the heart, and turns the course of it—and this forsooth is fever."

For many centuries after Galen, men were content to worship his name and his doctrines, and forsook his method. They did not follow the way of experiment, and invented theories that were no help either in science or in practice. Here, in Galen's observation of living arteries, was a great opportunity for physiology; but the example that he set to those who came after him was forgotten by them, and, from the time of Galen to the time of the Renaissance, physiology remained almost where he had left it. Of the men of the Renaissance, Servetus, Cæsalpinus, Ruinius, and others, Harvey's near predecessors, this much only need be said here, that they did not discover the circulation of the blood; and that the claim made a few years ago to this discovery, on behalf of Cæsalpinus, by his countrymen, was not successful. But it is probable that Realdus (1516-1557) did understand the passage of blood through the lungs, but not the general circulation. He says:—

"The blood is carried through the pulmonary artery to the lung, and there is attenuated; thence, mixed with air, it is carried through the pulmonary vein to the left ventricle of the heart: which thing no man hitherto has noted or left on record, though it is most worthy of the observation of all men.... And this is as true as truth itself; for if you will look, not only in the dead body but also in the living animal, you will always find this pulmonary vein full of blood, which assuredly it would not be if it were designed only for air and vapours.... Verily, I pray you, O candid reader, studious of authority, but more studious of truth, to make experiment on animals. You will find the pulmonary vein full of blood, not air or *fuligo*, as these men call it, God help them. Only there is no pulsation in the vein." (*De Re Anatomicâ*, Venice, 1559.)

Fabricius ab Aquapendente, Harvey's master at Padua, published his work on the valves of the veins—*De Venarum Ostiolis*—in 1603. He did not discover them. Sylvius speaks of them in his *Isagoge* (Venice, 1555), and they were known to Amatus (1552), and even to Theodoretus, Bishop of Syria, who lived, as John Hunter said of Sennertus, "the Lord knows how long ago." But Fabricius studied them most carefully; and in anatomy he left nothing more to be said about them. In physiology, his work was of little value; for he held that they were designed "to retard the blood in some measure, lest it should run pell-mell into the feet, hands, and fingers, there to be impacted": they were to prevent distension of the veins, and to ensure the due nourishment of all parts of the body. It is true that he compared them to the locks or weirs of a river, but he understood neither the course nor the force of the blood: as Harvey said of him, "The man who discovered these valves did not understand their right use; neither did they who came after him"—*Harum valvularum usum rectum inventor* *non est assecutus, nec alii addiderunt; non est enim ne pondere deorsum sanguis in inferiora totus ruat; sunt namque in jugularibus deorsum spectantes, et sanguinem sursum ferri prohibentes.* Men had no idea of the rapidity and volume of the circulation; they thought of a sort of Stygian tide, oozing this way or that way in the vessels—Cæsalpinus was of opinion that it went one way in the daytime and another at night—nor did they see that the pulmonary circulation and the general circulation are one system, the same blood covering the whole course. The work that they did in anatomy was magnificent; Vesalius, and the other great anatomists of his time, are unsurpassed. But physiology had been hindered for ages by fantastic imaginings, and the facts of the circulation of the blood were almost as far from their interpretation in the sixteenth century as they had been in the time of Galen.

II.—Harvey (1578-1657)

The *De Motu Cordis et Sanguinis in Animalibus* was published at Frankfurt in 1628. And it begins with these words: *Cum multis vivorum dissectionibus, uti ad manum dabantur*:—

"When by many dissections of living animals, as they came to hand, I first gave myself to observing how I might discover with my own eyes, and not from books and the writings of other men, the use and purpose of the movement of the heart in animals, forthwith I found the matter hard indeed, and full of difficulty: so that I began to think, with Frascatorius, that the movement of the heart was known to God alone. For I could not distinguish aright either the nature of its systole and diastole, or when or where dilatation and contraction took place; and this because of the swiftness of the movement, which in many animals in the twinkling of an eye, like a flash of lightning, revealed itself to sight and then was gone; so that I came to believe that I saw systole and diastole now this way now the other, and movements now apart and now together. Wherefore my mind wavered; I had nothing assured to me, whether decided by me or taken from other men: and I did not wonder that Andreas Laurentius had written that the movement of the heart was what the ebb and flow of the Euripus had been to Aristotle.

"At last, having daily used greater disquisition and diligence, by frequent examination of many and various living animals—*multa frequenter et varia animalia viva introspiciendo*—and many observations put together, I came to believe that I had succeeded, and had escaped and got out of this labyrinth, and therewith had discovered what I desired, the movement and use of the heart and the arteries. And from that time, not only to my friends, but also in public in my anatomical lectures, after the manner of the Academy, I did not fear to set forth my opinion in this matter."

It is plain, from Harvey's own words, that he gives to experiments on animals a foremost place among his methods of work. Take only the headings of his first four chapters:—

i. *Causæ, quibus ad scribendum auctor permotus fuerit.*
ii. *Ex vivorum dissectione, qualis fit cordis motus.*
iii. *Arteriarum motus qualis, ex vivorum dissectione.*

iv. *Motus cordis et auricularum qualis, ex vivorum dissectione.*

He thrusts it on us, he puts it in the foreground. Read the end of his Preface:—

"Therefore, from these and many more things of the kind, it is plain (since what has been said by men before me, of the movement and use of the heart and arteries, appears inconsistent or obscure or impossible when one carefully considers it) that we shall do well to look deeper into the matter; to observe the movements of the arteries and the heart, not only in man, but in all animals that have hearts; and by frequent dissection of living animals, and much use of our own eyes, to discern and investigate the truth—*vivorum dissectione frequenti, multâque autopsiâ, veritatem discernere et investigare*."

Finally, take the famous passage in the eighth chapter, *De copiâ sanguinis transeuntis per cor e venis in arterias, et de circulari motu sanguinis*:—

"And now, as for the great quantity and forward movement of this blood on its way, when I shall have said what things remain to be said—though they are well worth considering, yet they are so new and strange that I not only fear harm from the envy of certain men, but am afraid lest I make all men my enemies; so does custom, or a doctrine once imbibed and fixed down by deep roots, like second nature, hold good among all men, and reverence for antiquity constrains them. Be that as it may, the die is cast now: my hope is in the love of truth, and the candour of learned minds. I bethought me how great was the quantity of this blood. Both from the dissection of living animals for the sake of experiment, with opening of the arteries, with observations manifold; and from the symmetry of the size of the ventricles, and of the vessels entering and leaving the heart—because Nature, doing nothing in vain, cannot in vain have given such size to these vessels above the rest—and from the harmonious and happy device of the valves and fibres, and all other fabric of the heart; and from many other things—when I had again and again carefully considered it all, and had turned it over in my mind many times—I mean the great quantity of the blood passing through, and the swiftness of its passage—and I did not see how the juices of the food in the stomach could help the veins from being emptied and drained dry, and the arteries contrariwise from being ruptured by the excessive flow of blood into them, unless blood were always getting round from the arteries into the veins, and so back to the right ventricle—I began to think to myself whether the blood had a certain movement, as in a circle—*cœpi egomet mecum cogitare, an motionem quandam quasi in circulo haberet*—which afterward I found was true."

This vehement passage, which goes with a rush like that of the blood itself, is a good example of the width and depth of Harvey's work—how he used all methods that were open to him. He lived to fourscore years; "an old man," he says, "far advanced in years, and occupied with other cares": and, near the end of his life, he told the Hon. Robert Boyle that the arrangement of the valves of the veins had given him his first idea of the circulation of the blood:—

"I remember that when I asked our famous Harvey, in the only discourse I had with him, which was but a while before he died, what were the things which induced him to think of the circulation of the blood, he answered me that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave free passage of the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was invited to imagine that so provident a cause as Nature had not so placed so many valves without design; and no design seemed more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent by the arteries, and return through the veins, whose valves did not oppose its course that way."

But between this observation, which "invited him to imagine" a theory, and his final proofs of the circulation, lay a host of difficulties; and it is certain, from his own account of his work, that experiments on animals were of the utmost help to him in leading him "out of the labyrinth."

III.—After Harvey

1. *The Capillaries*

The capillary vessels were not known in Harvey's time: the *capillamenta* of Cæsalpinus were not the capillaries, but the νεῦρα (Greek: neura) of Aristotle. It was believed that the blood, between the smallest arteries and the smallest veins, made its way through "blind porosities" in the tissues, as water percolates through earth or through a sponge. The first account of the capillaries is in two letters (*De Pulmonibus*, 1661) from Malpighi, professor of medicine at Bologna, to Borelli, professor of mathematics at Pisa. In his first letter, Malpighi writes that he has tried in vain, by injecting the dead body, to discover how the blood passes from the arteries into the veins:—

"This enigma hitherto distracts my mind, though for its solution I have made many and many attempts, all in vain, with air and various coloured fluids. Having injected ink with a syringe into the pulmonary artery, I have again and again seen it escape (become extravasated into the tissues) at several points. The same thing happens with an injection of mercury. These experiments do not give us the natural pathway of the blood."

But, in his second letter, he describes how he has examined, with a microscope of two lenses, the lung and the mesentery of a frog, and has seen the capillaries, and the blood in them:—

"Such is the divarication of these little vessels, coming off from the vein and the artery, that the order in which the vessel ramifies is no longer preserved, but it looks like a network woven from the offshoots of both vessels."

He was able, in a dead frog, to see the capillaries; and then, in a living frog, to see the blood moving in them. But, in spite of this work, it took nearly half a century before Harvey's teaching was believed by all men—*Tantum consuetudo apud omnes valet*.

2. *The Blood-pressure*

Harvey had seen the facts of blood-pressure—*the great quantity of blood passing through, and the swiftness of its passage*—but he had not measured it. Keill's experiments on the blood-pressure (1718) were inexact, and of no value; and the first exact measurements were made by Stephen Hales, who was rector of Farringdon, Hampshire, and minister of Teddington, Middlesex; a Doctor of Divinity, and a Fellow of the Royal Society. His experiments, in their width and diversity, were not surpassed even by those of John Hunter, and were extended far over physiology, vegetable physiology, organic and inorganic chemistry, and physics; they ranged from the invention of a sea-gauge to the study of solvents for the stone, and he seems to have experimented on every force in Nature. The titles of his two volumes of *Statical Essays* (1726-1733) show the great extent of his non-clerical work:—

Volume I. *Statical Essays, containing Vegetable Statics, or an Account of some Statical Experiments on the Sap in Vegetables, being an Essay towards a Natural History of Vegetation; also, a Specimen of an Attempt to Analyse the Air, by a great Variety of Chymio-Statical Experiments.*

Volume II. *Statical Essays, containing Hæmostatics, or an Account of some Hydraulic and Hydrostatical Experiments made on the Blood and Blood-vessels of Animals; also, an Account of some Experiments on Stones in the Kidneys and Bladder, with an Enquiry into the Nature of those anomalous Concretions.*

"We can never want matter for new experiments," he says in his preface. "We are as yet got little further than to the surface of things: we must be content, in this our infant state of knowledge, while we know in part only, to imitate children, who, for want of better skill and abilities, and of more proper materials, amuse themselves with slight buildings. The farther advances we make in the knowledge of Nature, the more probable and the nearer to truth will our conjectures approach: so that succeeding generations, who shall have the benefit and advantage both of their own observations and those of preceding generations, may then make considerable advances, when *many shall run to and fro, and knowledge shall be increased*."

His account of his plan of measuring the blood-pressure, and of one of many experiments that he made on it, is as follows:—

"Finding but little satisfaction in what had been attempted on this subject by Borellus and others, I endeavoured, about twenty-five years since, by proper experiments, to find what was the real force of the blood in the crural arteries of dogs, and about six years afterwards I repeated the like experiments on two horses, and a fallow doe; but did not then pursue the matter any further, being discouraged by the disagreeableness of anatomical dissections. But having of late years found by experience the advantage of making use of the statical way of investigation, not only in our researches into the nature of vegetables, but also in the chymical analysis of the air, I was induced to hope for some success, if the same method of enquiry were applied to animal bodies....

"Having laid open the left crural artery (of a mare), I inserted into it a brass pipe whose bore was 1/6 of an inch in diameter; and to that, by means of another brass pipe which was fitly adapted to it, I fixed a glass tube of nearly the same diameter, which was 9 feet in length; then, untying the ligature on the artery, the blood rose in the tube 8 feet 3 inches perpendicular above the level of the left ventricle of the heart, but it did not attain to its full height at once: it rushed up gradually at each pulse 12, 8, 6, 4, 2, and sometimes 1 inch. When it was at its full height, it would rise and fall at and after each pulse 2, 3, or 4 inches, and sometimes it would fall 12 or 14 inches, and have there for a time the same vibrations up and down, at and after each pulse, as it had when it was at its full height, to which it would rise again, after forty or fifty pulses."

3. *The Collateral Circulation*

After Hales, came John Hunter, who was five years old when the *Statical Essays* were published. His experiments on the blood were mostly concerned with its properties, not with its course; but one great experiment must be noted here that puts him in line with Harvey, Malpighi, and Hales. He got from it his knowledge of the collateral circulation; he learned how the obstruction of an artery is followed by enlargement of the vessels in its neighbourhood, so that the parts beyond the obstruction do not suffer from want of blood: and the facts of collateral circulation were fresh in his mind when, a few months later, he conceived and performed his operation for aneurysm (December 1785). The "old operation" gave him no help here; and "Anel's operation" was but a single instance, and no sure guide for Hunter, because Anel's patient had a different sort of aneurysm. Hunter knew that the collateral circulation could be trusted to nourish the limb, if the femoral artery were ligatured in "Hunter's canal" for the cure of popliteal aneurysm; and he got this knowledge from the experiment that he had made on one of the deer in Richmond Park, to see the influence of ligature of the carotid artery on the growth of the antler. The following account of this experiment was given by Sir Richard Owen, who had it from Mr. Clift, Hunter's devoted pupil and friend:—

"In the month of July, when the bucks' antlers were half-grown, he caused one of them to be caught and thrown; and, knowing the arterial supply to the hot 'velvet,' as the keepers call it, Hunter cut down upon and tied the external carotid; upon which, laying his hand upon the antler, he found that the pulsations of the arterial channels stopped, and the surface soon grew cold. The buck was released, and Hunter speculated on the result—whether the antler, arrested at mid-growth, would be shed like the full-grown one, or be longer retained. A week or so afterward he drove down again to the park, and caused the buck to be caught and thrown. The wound was healed about the ligature; but on laying his hand on the antler, he found to his surprise that the warmth had returned, and the channels of supply to the velvety formative covering were again pulsating. His first impression was that his operation had been defective. To test this, he had the buck killed and sent to Leicester Square. The arterial system was injected. Hunter found that the external carotid had been duly tied. But certain small branches, coming off on the proximal or heart's side of the ligature, had enlarged; and, tracing-on these, he found that they had anastomosed with other small branches from the distal continuation of the carotid, and these new channels had restored the supply to the growing antler.... Here was a consequence of his experiment he had not at all foreseen or expected. A new property of the living arteries was unfolded to him."

All the anatomists had overlooked this physiological change in the living body, brought about by disease. And the surgeons, since anatomy could not help them, had been driven by the mortality of the "old operation" to the practice of amputation.

4. *The Mercurial Manometer*

Hale's experiments on the blood-pressure were admirable in their time; but neither he nor his successors could take into account all the physiological and mathematical facts of the case. But a great advance was made in 1828, when Poiseuille published his thesis, *Sur la Force du Cœur Aortique*, with a description of the mercurial manometer. Poiseuille had begun with the received idea that the blood-pressure in the arteries would vary according to the distance from the heart, but he found by experiment that this doctrine was wrong:—

"At my first experiments, wishing to make sure whether the opinions, given *à priori*, were true, I observed to my great astonishment that two tubes, applied at the same time to two arteries at different distances from the heart, gave columns of exactly the same height, and not, as I had expected, of different heights. This made the work very much simpler, because, to whatever artery I applied the instrument, I obtained the same results that I should have got by placing it on the ascending aorta itself."

He found also, by experiments, that the coagulation of the blood in the tube could be prevented by filling one part of the tube with a saturated solution of sodium carbonate. The tube, thus prepared, was connected with the artery by a fine cannula, exactly fitting the artery. With this instrument, Poiseuille was able to obtain results far more accurate than those of Hales, and to observe the diverse influences of the respiratory movements on the blood-pressure. He sums up his results in these words:—

"I come to this irrevocable conclusion, that the force with which a molecule of blood moves, whether in the carotid, or in the aorta, etc., is exactly equal to the force which moves a molecule in the smallest arterial branch; or, in other words, that a molecule of blood moves with the same force over the whole course of the arterial system—which, *à priori*, with all the physiologists, I was far from thinking."

And he adds, in a footnote:—

"When I say that this force is the same over the whole course of the arterial system, I do not mean to deny that it must needs be modified at certain points of this system, which present a special arrangement, such as the anastomosing arches of the mesentery, the arterial circle of Willis, etc."

Later, in 1835, he published a very valuable memoir on the movement of the blood in the capillaries under different conditions of heat, cold, and atmospheric pressure.

5. *The Registration of the Blood-pressure*

Poiseuille's work, in its turn, was left behind as physiology went forward: especially, the discovery of the vaso-motor nerves compelled physiologists to reconsider the whole subject of the blood-pressure. If Poiseuille's thesis (1828) be compared with Marey's book (1863), *Physiologie Médicale de la Circulation du Sang*, it will be evident at once how much wider and deeper the problem had become. Poiseuille's thesis is chiefly concerned with mathematics and hydrostatics; it suggests no method of immediate permanent registration of the pulse, and is of no great value to practical medicine: Marey's book, by its very title, shows what a long advance had been made between 1828 and 1863—*Physiologie Médicale de la Circulation du Sang, basée sur l'étude graphique des mouvements du cœur et du pouls artériel, avec application aux maladies de l'appareil circulatoire*. Though the contrast is great between Hales' may-pole and Poiseuille's manometer, there is even a greater contrast between Poiseuille's mathematical calculations and Marey's practical use of the sphygmograph for the study of the blood-pressure in health and disease. Marey had the happiness of seeing medicine, physiology, and physics, all three of them working to one end:—

"La circulation du sang est un des sujets pour lesquels la médecine a le plus besoin de s'éclairer de la physiologie, et où celle-ci à son tour tire le plus de lumière des sciences physiques. Ces dernières années sont marquées par deux grands progrès qui ouvrent aux recherches à venir des horizons nouveaux: en Allemagne, l'introduction des procédés graphiques dans l'étude du mouvement du sang; en France, la démonstration de l'influence du système nerveux sur la circulation périphérique. Cette dernière découverte, que nous devons à M. Cl. Bernard, et qui depuis dix ans a donné tant d'impulsion à la science, montre mieux que toute autre combien la physiologie est indispensable à la médecine, tandis que les travaux allemands ont bien fait ressortir l'importance des connaissances physiques dans les études médicales."

Marey's sphygmograph was not the first instrument of its kind. There had been, before it, Hérisson's sphygmometer, Ludwig's kymographion, and the sphygmographs of Volckmann, King, and Vierordt. But, if one compares a Vierordt tracing with a Marey tracing, it will be plain that Marey's results were far advanced beyond the useless "oscillations isochrones" recorded by Vierordt's instrument.

Beside this improved sphygmograph, Chauveau and Marey also invented the cardiograph, for the observation of the blood-pressure within the cavities of the heart. Their cardiograph was a set of very delicate elastic tambours, resting on the heart, or passed through fine tubes into the cavities of the heart,[1] and communicating impulses to levers with writing-points. These writing-points, touching a revolving cylinder, recorded the variations of the endocardial pressure, and the duration of the auricular and ventricular contractions.

It is impossible here to describe the subsequent study of those more abstruse problems that the older physiologists had not so much as thought of: the minutest variations of the blood-pressure, the multiple influences of the nervous system on the heart and blood-vessels, the relations between blood-pressure and secretion, the automatism of the heart-beat, the influence of gravitation, and other finer and more complex issues of physiology. But, even if one stops at Marey's book, now more than forty years old, there is an abundant record of good work, from the discovery of the circulation to the invention of the sphygmograph.

II
THE LACTEALS

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Asellius, in his account of his discovery of the lacteal vessels (1622), is of opinion that certain of "the ancients" had seen these vessels, but had not recognised them. He has a great reverence for authority: Hippocrates, Plato, Aristotle, the Stoics, Herophilus, Galen, Pollux, Rhases, and a host of other names, he quotes them all, and all with profound respect; and comes to this conclusion: "It did not escape the ancients, that certain vessels must needs be concerned with containing and carrying the chyle, and certain other vessels with the blood: but the true and very vessels of the chyle, that is, my 'veins,' though they were seen by some of the ancients, yet they were recognised by none of them." He can forgive them all, except Galen, *qui videtur nosse omnino debuisse*—"but, as for Galen, I know not at all what I am to think. For he, who made more than six hundred sections of living animals, as he boasts himself, and so often opened many animals when they were lately fed, are we to think it possible that these veins never showed themselves to him, that he never had them under his eyes, that he never investigated them—he to whom Erasistratus had given so great cause for searching out the whole matter?" Probably, the milk-white threads had been taken for nerves by those who had seen them: and those who had never seen them, but believed in their existence, rested their belief on a general idea that the chyle must, somehow, have vessels of its own apart from the blood-vessels. What Galen and Erasistratus must have seen, Asellius and Pecquet discovered: and Harvey gives a careful review of the discovery in his letters to Nardi (May 1652) and to Morison (November 1653). He does not accept it; but the point is that he recognises it as a new thing altogether.

A year or two after he had made the discovery, Asellius died; and his work was published in 1627 by two Milanese physicians, and was dedicated by them to the senate of the Academy of Milan, where Asellius had been professor of anatomy. The full title of his book is, *De Lactibus sive Lacteis Venis, quarto Vasorum Mesaraicorum genere novo invento, Gasparis Asellii Cremonensis, Anatomici Ticinensis, Dissertatio. Quâ sententiæ anatomicæ multæ vel perperam receptæ convelluntur vel partim perceptæ illustrantur.* He gives the following account of the discovery, in the chapter entitled *Historia primæ vasorum istorum inventionis cum fide narrata*. On 23rd July 1622, demonstrating the movement of the diaphragm in a dog, he observed suddenly, "as it were, many threads, very thin and very white, dispersed through the whole mesentery and through the intestines, with ramifications almost endless"—*plurimos, eosque tenuissimos candido-sissimosque ceu funiculos per omne mesenterium et per intestina infinitis propemodum propaginibus dispersos*:—

"Thinking at first sight that they were nerves, I did not greatly heed them. But soon I saw that I was wrong, for I bethought me that the nerves, which belong to the intestines, are distinct from these threads, and very different from them, and have a separate course. Wherefore, struck by the newness of the matter, I stopped for a time silent, while one way and another there came to my mind the controversies that occupy anatomists, as to the mesenteric veins and their use; which controversies are as full of quarrels as of words. When I had pulled myself together, to make experiment, taking a very sharp scalpel, I pierce one of the larger threads. Scarcely had I hit it off, when I see a white fluid running out, like milk or cream. At which sight, when I could not hold my joy, turning to those who were there, first to Alexander Tadinus and Senator Septalius, both of them members of the most honourable College of Physicians, and, at the time of this writing, officers of the public health, '*I have found it*,' I say like Archimedes; and therewith invite them to the so pleasant sight of a thing so unwonted; they being agitated, like myself, by the newness of it."

He then describes the collapse and disappearance of the vessels at death, and the many experiments which he made for further study of them; and the failure, when he tried to find them in animals not lately fed. He did not trace them beyond the mesentery, and believed that they emptied themselves into the liver. The discovery of their connection with the receptaculum chyli and the thoracic duct was made by Jehan Pecquet of Dieppe, Madame de Sévigné's doctor, her "good little Pecquet." The full title of his book (2nd ed., 1654) is, *Expérimenta Nova Anatomica, quibus incognitum hactenus Receptaculum, et ab eo per Thoracem in ramos usque subclavios Vasa Lactea deteguntur*. He has not the academical learning of Asellius, nor his obsequious regard for the ancients; and the discovery of the thoracic duct came, as it were by chance, out of an experiment that was of itself wholly useless. He had killed an animal by removing its heart, and then saw a small quantity of milky fluid coming from the cut end of the vena cava—*Albicantem subinde Lactei liquoris, nec certe parum fluidi scaturiginem, intra Venæ Cavæ fistulam, circ[=a] dextri sedem Ventriculi, miror effluere*—and found that this fluid was identical with the chyle in the lacteals. In another experiment, he succeeded in finding the thoracic duct—"At last, by careful examination deep down along the sides of the dorsal vertebræ, a sort of whiteness, as of a lacteal vessel, catches my eyes. It lay in a sinuous course, close up against the spine. I was in doubt, for all my scrutiny, whether I had to do with a nerve or with a vessel. Therefore, I put a ligature a little below the clavicular veins; and then the flaccidity above the ligature, and the swelling of the distended duct below the ligature, broke down my doubt—*Ergo subducto paulo infra Claviculas vinculo, cum a ligaturâ sursum flaccesceret, superstite deorsum turgentis alveoli tumore, dubium meum penitus enervavit.... Laxatis vinculis, lacteus utrinque rivulus in Cavam affatim Chylum profudit.*"

It is to be noted that Asellius and Pecquet, both of them, made their discoveries as it were by chance. Unless digestion were going on, the lacteals would be empty and invisible; and, on the dead body, lacteals, receptaculum, and thoracic duct would all be empty. For these reasons, it cost a vast number of experiments to prove the existence, and to discover the course, of these vessels. Once found in living animals, they could be injected and dissected in the dead body; but they had been overlooked by Vesalius and the men of his time.

From the discovery of the lacteals came the discovery of the whole lymphatic system. Daremberg, in his *Histoire des Sciences Médicales* (Paris, 1870), after an account of Pecquet's work, says:—

"Up to this point, we have seen English, Italians, and French working together, with more or less success and genius, to trace the true ways of blood and chyle: there is yet one field of work to open up, the lymphatics of the body. The chief honour here belongs, without doubt, to the Swede Rudbeck, though the Dane Bartholin has disputed it with him, with equal acrimony and injustice."

Rudbeck's work (1651-54) coincides exactly, in point of time, with the first and second editions, 1651 and 1654, of Pecquet's *De Lactibus*. It may be said, therefore, that the whole doctrine of the lymphatic system was roughed out half-way through the seventeenth century.

III
THE GASTRIC JUICE

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From many causes, the experimental study of the digestive processes came later than the study of the circulation. As an object of speculative thought, digestion was a lower phase of life, the work of crass spirits, less noble than the blood; from the point of view of science, it could not be studied ahead of organic chemistry, and got no help from any other sort of knowledge; and, from the medical point of view, it was the final result of many unknown internal forces that could not be observed or estimated either in life or after death. It did not, like the circulation, centre itself round one problem; it could not be focussed by the work of one man. For these reasons, and especially because of its absolute dependence on chemistry for the interpretation of its facts, it had to bide its time; and Réaumur's experiments are separated from the publication of Harvey's *De Motu Cordis et Sanguinis* by a hundred and thirty years.

The following account of the first experiments on digestion is taken from Claude Bernard's *Physiologie Opératoire*, 1879:—

"The true experimental study of digestion is of comparatively recent date; the ancients were content to find comparisons, more or less happy, with common facts. Thus, for Hippocrates, digestion was a 'coction': for Galen, a 'fermentation,' as of wine in a vat. In later times, van Helmont started this comparison again: for him, digestion was a fermentation like that of bread: as the baker, having kneaded the bread, keeps a little of the dough to leaven the next lot kneaded, so, said van Helmont, the intestinal canal never completely empties itself, and the residue that it keeps after each digestion becomes the leaven that shall serve for the next digestion.

"The first experimental studies on the digestion date from the end of the seventeenth century, when the Academy of Florence was the scene of a famous and long controversy between Borelli and Valisnieri. The former saw nothing more in digestion than a purely mechanical act, a work of attrition whereby the ingesta were finely divided and as it were pulverised: and in support of this opinion Borelli invoked the facts that he had observed relating to the gizzard of birds. We know that this sac, with its very thick muscular walls, can exercise on its contents pressure enough to break the hardest bodies. Identifying the human stomach with the bird's gizzard, Borelli was led to attribute to the walls of the stomach an enormous force, estimated at more than a thousand pounds; whose action, he said, was the very essence of digestion. Valisnieri, on the contrary, having had occasion to open the stomach of an ostrich, had found there a fluid which seemed to act on bodies immersed in it; this fluid, he said, was the active agent of digestion, a kind of *aqua fortis* that dissolved food.

"These two opposed views, resulting rather from observations than from regularly instituted experiments, were the starting-point of the experimental researches undertaken by Réaumur in 1752. To resolve the problem set by Borelli and Valisnieri, Réaumur made birds swallow food enclosed in fenestrated tubes, so that the food, protected from the mechanical action of the walls of the stomach, was yet exposed to the action of the gastric fluid. The first tubes used (glass, tin, etc.) were crushed, bent, or flattened by the action of the walls of the gizzard; and Réaumur failed to oppose to this force a sufficient resistance, till he employed leaden tubes thick enough not to be flattened by a pressure of 484 pounds: which was, in fact, the force exercised by the contractile walls of the gizzard in turkeys, ducks, and fowls under observation. These leaden tubes—filled with ordinary grain, and closed only by a netting that let pass the gastric juices—these tubes, after a long stay in the stomach, still enclosed grain wholly intact, unless it had been crushed before the experiment. When they were filled with meat, it was found changed, but not digested. Réaumur was thus led at first to consider digestion, in the gallinaceæ, as pure and simple trituration. But, repeating these experiments on birds of prey, he observed that digestion in them consists essentially in dissolution, without any especial mechanical action, and that it is the same with the digestion of meat in all animals with membranous stomachs. To procure this dissolving fluid, Réaumur made the birds swallow sponges with threads attached: withdrawing these sponges after a definite period, he squeezed the fluid into a glass, and tested its action on meat. That was the first attempt at artificial digestion *in vitro*. He did not carry these last investigations very far, and did not obtain very decisive results; nevertheless he must be considered as the discoverer of artificial digestion."

After Réaumur, the Abbé Spallanzani (1783) made similar observations on many other animals, including carnivora. He showed that even in the gallinaceæ there was dissolution of food, not mere trituration: and observed how after death the gastric fluid may under certain conditions act on the walls of the stomach itself.

"Henceforth the experimental method had cut the knot of the question raised by the theories of Borelli and Valisnieri: digestion could no longer be accounted anything but a dissolution of food by the fluid of the stomach, the gastric juice. But men had still to understand this gastric juice, and to determine its nature and mode of action. Nothing could be more contradictory than the views on this matter. Chaussier and Dumas, of Montpellier, regarded the gastric juice as of very variable composition, one time alkaline, another acid, according to the food ingested. Side by side with these wholly theoretical opinions, certain results of experiments had led to ideas just as erroneous, for want of rigorous criticism of methods; it was thus that Montègre denied the existence of the gastric juice as a special fluid; what men took for gastric juice, he said, was nothing but the saliva turned acid in the stomach. To prove his point, he made the following experiment:—He masticated a bit of bread, then put it out on a plate; it was at first alkaline, then